

XXXII. *An Account of some Experiments made with an Air-pump on Mr. Smeaton's Principle; together with some Experiments with a common Air-pump.* By Mr. Edward Nairne, F. R. S.

Read June 12,
1777.

AS the following experiments were made principally to try the performance of Mr. SMEATON's pear-gage, it may be proper to describe it, which I shall do in his own words, taken from the Phil. Transf. for the years 1751 and 1752, vol. XLVII. p. 420.

"I have found," says Mr. SMEATON, "the gages that have been hitherto made use of, for measuring the expansion of the air, very unfit to determine in an experiment of so much nicety; I have therefore contrived one of a different sort, which measures the expansion with certainty to much less than the 1000th part of the whole. It consists of a bulb of glass, something in the shape of a pear, and sufficient to hold
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“ about half a pound of quicksilver: it is open at one
“ end, and at the other is a tube, hermetically closed
“ at the top. By the help of a nice pair of scales I found
“ what proportion of weight a column of quicksilver, of
“ a certain length, contained in the tube, bore to that
“ which filled the whole vessel: by these means I was
“ enabled to mark divisions upon the tube answering to
“ a 1000th part of the whole capacity; which being
“ about one-tenth of an inch each, may, by estimation,
“ be easily sub-divided into smaller parts. This gage,
“ during the exhausting of the receiver, is suspended
“ therein by a slip wire. When the pump is worked as
“ much as shall be thought necessary, the gage is pushed
“ down till the open end is immersed in a cistern of
“ quicksilver placed underneath; the air being then let
“ in, the quicksilver will be driven into the gage till the
“ air remaining in it becomes of the same density with
“ the external, and as the air always takes the highest
“ place, the tube being uppermost, the expansion will be
“ determined by the number of divisions occupied by
“ the air at the top.

“ The degree to which I have been able to rarify the
“ air in an experiment, has generally been about 1000
“ times, when the pump is put clean together; but the

“moisture that adheres to the inside of the barrel as
“well as other internal parts, upon letting in the air, is
“in the same succeeding trials worked together with the
“oil, which soon renders it so clammy as to obstruct
“the actions of the pump upon a fluid so subtil as the
“air when so much expanded; but in this case it seldom
“fails to act upon the air in the receiver till it is ex-
“panded 500 times.” Thus far Mr. SMEATON’S ac-
count.

The pump with which the chief of the following experiments were made, had the leather of its piston soaked in oil and tallow (and oil in the barrel) and every precaution was taken that no water should get into the working parts of the pump, except what might arise in vapour from the substances which were under the receiver.

EXPERIMENT I.

Having provided a pear-gage agreeable to Mr. SMEATON’S description, on which the space of a 4000th part of the whole capacity was two-tenths of an inch; this gage, together with a glass cup which served as a cistern
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to hold the quicksilver for it, was put under a receiver; which receiver was placed on a leather soaked in oil and tallow, on the plate of the pump.

I must observe here, that the foot of the glass cup, which held the quicksilver for the pear-gage, being broken off by an accident, another foot, made of a piece of box-wood, was cemented to it. The reason of mentioning this circumstance, which may seem trifling, will appear in the sixth experiment. The pump was then worked, and the pear-gage was pushed down till its open end was immersed in the quicksilver in the cup: the air being then let into the receiver, it forced the quicksilver into the gage till it was filled within a 4000th part of the whole, so that by this gage the pump appeared to have expanded the air 4000 times. To what it might be owing that this pump, which was on Mr. SMEATON's principle, should expand the air 4000 times instead of only 1000 as Mr. SMEATON's, I could not even surmise.

Having the pleasure of knowing Mr. SMEATON, and being well acquainted with his great abilities in practice as well as theory, I could not imagine that this apparent superiority could proceed from our having executed the various parts of the pump in a more perfect manner than

he had done. I therefore determined, for greater certainty, to see if the fault might not be in the gages; and for that purpose I repeated the experiment with the syphon-gage, and both the long and short barometer-gages, and found that the several degrees of exhaustion indicated by these, were very different from that which the pear-gage had indicated: no conclusion, therefore, could be drawn from this experiment.

I determined next to compare again this pear-gage with the long and short barometer-gages with all the accuracy I was master of: and first, lest the tubes of these barometer-gages might not be perfectly clean and free from moisture, I had some tubes made at the glass house; and as soon as they were brought home, which was within an hour after they were made, two of them, which were of the same size, were filled with distilled quicksilver; and then the quicksilver was carefully boiled in the tubes the whole length, which was about thirty-six inches: I then cut off about six inches from the sealed end of one of the tubes, and took care to keep it perfectly full of the boiled quicksilver; it was then inverted into a glass cistern containing boiled quicksilver; and a piece of very thin ivory, about half an inch in length, with divisions on its edge, was put over the tube, so as to float on the surface
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of the quicksilver in the cistern, by which means the difference of the two surfaces could be seen to a great nicety. This kind of gage is called the short barometer-gage.

The other tube, which was cut off to thirty-three inches, being perfectly full of the boiled quicksilver, was also carefully inverted into a glass cistern containing boiled quicksilver, to such a depth that from the surface of the quicksilver in the cistern to the top of the tube was twenty-nine inches; this had likewise a piece of ivory, with divisions on it, put over the tube, so as to float on the surface of the quicksilver in the cistern, in the same manner as the other.

This long tube and the short barometer-gage being put both of them at the same time under the receiver, which was carefully cemented to the plate of the pump; the pump was then worked for ten minutes, and the surface of the quicksilver in both the tubes came down very nearly to within one-twentieth or five hundredth parts of an inch of the surface of the quicksilver in their respective cisterns.

The air was then let in, and the receiver being taken from the pump, the long tube was raised up so far in the cistern as to let the quicksilver come down from the top of the tube, so that it now became a common barometer,

and its height from the surface of the quicksilver in the cistern measured thirty inches; which agreed exactly with an open cistern-barometer I had in the room. The quicksilver in the tube of this barometer was also boiled, and the measure the same.

The quicksilver was then emptied out of this long tube, and the sealed end being cut off, it was then cemented to a piece of brass, by which means it was screwed to the air-pump; and the lower end being immersed in a cup of boiled quicksilver, it then made that kind of gage where the air is taken from the top of the tube, and which is called the long barometer-gage. This gage being fixed to the pump, and the short barometer-gage put on the plate of the pump under a receiver, the receiver was cemented to the plate of the pump, and the pump worked for ten minutes as before. The quicksilver in the short barometer-gage fell now nearly to within one-twentieth or five hundredth parts of an inch of that in the cistern, and the quicksilver in the long gage rose nearly to within a twentieth or five hundredth parts of an inch of the height it was at when it was made as a common barometer.

Gages made with these precautions seem to me to be the most to be depended upon, in determining the actual
diminution

diminution of the preffure on the furface of the quickfilver in the tube of the long gage, and alfo on the furface of the quickfilver in the ciftern of the fhort gage. But of thefe two gages the long barometer-gage was chiefly ufed in the following experiments, as being fixed to the pump: however, having now made thefe two gages with as much accuracy as I was mafter of, and finding that they agreed pretty nearly, I proceeded to repeat my firft experiment.

EXPERIMENT II.

I put the fhort barometer-gage, and the pear-gage with the glafs cup having a wooden foot, both together under the receiver, which receiver was placed on a leather foaked in oil and tallow on the plate of the pump; the pump was then worked for ten minutes, and the quickfilver was brought down in the fhort barometer-gage to about one-tenth of an inch of the furface of the quickfilver in the ciftern, and rofe in the long barometer-gage to within one-tenth of an inch of the height of the quickfilver in my ftandard barometer, which was at that time at thirty inches; by which it appeared, that
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the pressure on the surface of the quicksilver in the cistern, and in the tube of the long barometer-gage, was diminished to about a three-hundredth part: the pear-gage being now pushed down till its open end was immersed under the surface of the quicksilver in the cup, the air was then let in, and the pump appeared by that gage to have exhausted all but a six thousandth part of the air; or, in other words, the degree of exhaustion by this gage appeared to be six thousand times.

Finding still this disagreement between the pear-gage and the other gages, I tried a variety of experiments; but none of them appeared to me satisfactory, till one day in April 1776, shewing an experiment with one of these pumps to the honourable HENRY CAVENDISH, Mr. SMEATON, and several other gentlemen of the Royal Society, when the two gages differed some thousand times from one another, Mr. CAVENDISH accounted for it in the following manner. "It appeared," he said, "from some experiments of his father's, Lord CHARLES CAVENDISH, that water, whenever the pressure of the atmosphere on it is diminished to a certain degree, is immediately turned into vapour, and is as immediately turned back again into water on restoring the pressure. This degree of pressure is different according to the heat of the water: when the heat is 72° of FAH-
" RENHEIT'S

“ RENHEIT’s scale, it turns into vapour as soon as the
“ preffure is no greater than that of three quarters of an
“ inch of quickfilver, or about one-fortieth of the usual
“ preffure of the atmosphere; but when the heat is only
“ 41° , the preffure must be reduced to that of a quarter
“ of an inch of quickfilver before the water turns into
“ vapour. It is true, that water exposed to the open air
“ will evaporate at any heat, and with any preffure of
“ the atmosphere; but that evaporation is intirely owing
“ to the action of the air upon it: whereas the evapora-
“ tion here spoken of is performed without any assistance
“ from the air. Hence it follows, that when the receiver
“ is exhausted to the above-mentioned degree, the mois-
“ ture adhering to the different parts of the machine
“ will turn into vapour and supply the place of the air,
“ which is continually drawn away by the working of
“ the pump, so that the fluid in the pear-gage, as well as
“ that in the receiver, will consist in good measure of va-
“ pour. Now letting the air into the receiver, all the
“ vapour within the pear-gage will be reduced to water,
“ and only the real air will remain uncondensed; conse-
“ quently the pear-gage shews only how much real air
“ is left in the receiver, and not how much the preffure
“ or spring of the included fluid is diminished, whereas
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“ the common gages shew how much the pressure of
 “ the included fluid is diminished, and that equally,
 “ whether it consist of air or of vapour.”

Mr. CAVENDISH having explained so satisfactorily the cause of the disagreement between the two gages, I considered, that, if I were to avoid moisture as much as possible, the two gages should nearly agree: this induced me to make the following experiment.

EXPERIMENT III.

The plate of the pump being made as clean and as dry as possible, there was then put on it the before-mentioned short barometer-gage, also the pear-gage with a cistern entirely of glass which held the quicksilver; they were then covered with a receiver, round the outside of which was laid a cement which perfectly excluded the outward air; every part, before it was put under the receiver, as well as the receiver itself, being made as clean and as free from moisture as possible^(a). The pump was then worked for ten minutes,

(a) It may be proper here to take notice, that the pump in every experiment hereafter mentioned was worked ten minutes, and the same receiver continued cemented to the pump-plate, except where it is otherwise mentioned. The top part of this receiver was made to open, in order to put in different things.

and the barometer-gages indicated a degree of exhaustion nearly 600; the air was then let into the receiver, the pear-gage indicated a degree of exhaustion, but very little more than 600 also. The near agreement of the pear-gage with the barometer-gages in this last experiment, in which I had been so careful to exclude the moisture as much as possible, seemed to prove beyond a doubt, that their disagreeing in the first and second experiments must have been owing (as Mr. CAVENDISH supposed) to the moisture which in them had not been so carefully excluded. But I began now to suspect also, that there might arise a vapour from some moisture that might be contained in the leather soaked in oil and tallow, or in the wooden foot which was cemented to the glass cup, both used in the first and second experiments: these suspicions induced me to try the following experiments.

EXPERIMENT IV.

A piece of leather dressed in allum, known by the name of white sheep-skin, of about four inches diameter, which had been soaked in oil and tallow about a year ago (such as was used to place the receiver on in the first and second experiments) was put into the receiver; the

pump was then worked, and the barometer-gage indicated a degree of exhaustion of nearly 300; but on the admission of the air the pear-gage indicated a degree of exhaustion of 4000.

EXPERIMENT V.

The piece of leather being taken out, the pump was then worked, and the degree of exhaustion appeared by both the barometer and pear-gages to be about 600, as in the third experiment.

EXPERIMENT VI.

A cylinder made of a piece of box wood (which I had kept by me for more than a year) one inch in diameter and three inches in length, was put into the receiver (this piece of wood was of the same kind as that which was cemented to the foot of the glass cup used in the first and second experiments) the pump was then worked, and the degree of exhaustion appeared by the barometer-gage to be 300, but by the pear-gage 16,000.

These experiments have often been repeated, but the result was seldom the same. When leather soaked in oil and tallow has been put into the receiver, the pear-gage has sometimes indicated a degree of exhaustion of

20,000,

20,000, and sometimes no more than 500; it likewise differs very much with the box wood, which may perhaps be owing to different degrees of heat and moisture.

From these experiments it is evident, that there arises an elastic vapour from the leather dressed in allum and soaked in oil and tallow, and also from the piece of box wood, when the weight of the atmosphere has been partly taken off by the action of the pump; and that this vapour presses upon the surface of the quicksilver in the tube of the long barometer-gage, and of that in the cistern of the short one; and that, consequently, the testimony of both these gages must be influenced by this vapour, as well as by the small remainder of common air: but as it is the nature of the pear-gage not to give its testimony till the remaining air contained in it is pressed, so as to become of the same density of the atmosphere; and as this vapour cannot subsist in the form of vapour under that pressure, this gage is not at all influenced by it, but indicates the remaining quantity of permanent air only.

Seeing thus what a considerable quantity of vapour arose from the compound of leather, allum, oil, and tallow, my next object was to find out from which of those substances it chiefly arose; how far I have succeeded will appear by the following experiments.

Substances put into the receiver.	Weight when put into the receiver.	Degree of exhaustion according to	
		Barom. gage.	Pear-gage.
EXP. VII. Tallow, — —	2 ounces	431	600
EXP. VIII. Oil, — —	2 ounces	377	480.
EXP. IX. Alum, — —	2 ounces	378	580
EXP. X. A piece of leather as it came from the leather-sellers, — —	100 grains	152	100,000
EXP. XI. The same piece of leather soaked in the same two ounces of tallow and oil melted together, — —		432	800

From these experiments it appears, that the elastic vapour which caused so great a difference in the testimony of the gages, arose principally from the leather, and but little from the tallow, oil, or allum: it even appears by the tenth experiment, that it came from the leather, and supplied the place of the exhausted air so fast, that I could not (at least in the ten minutes) make the barometer-gage indicate a degree of exhaustion of more than 159.

To determine whether it was the moisture in the leather from which the vapour arose, I made the following experiments.

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Substances put into the receiver.	Weight when put into the receiver.	Degrees of exhaustion according to		Variation in weight during the experiments.
		Barom. gage.	Pear-gage.	
EXP. XII. A piece of white leather, fresh from the leather-sellers, — — —	100 grains	134	100,000	lost 2 grains.
EXP. XIII. The same piece of leather, dried by the fire till it would lose no more of its weight, — — —	80 grains	268	280	gained 2 grs.
EXP. XIV. The same piece of leather held in the steam of hot water till it had regained the 20 grains it had been deprived of, — — —	100 grains	147	100,000	lost 2 grains.

In this last experiment it was full three quarters of an hour before the leather regained the twenty grains of weight, although it was held very near the surface of the hot water.

Whenever I have asserted, that the degree of exhaustion, according to the pear-gage, was so great as 100,000, I only guessed it to be thereabouts, for my gage is not graduated to more than 4000; but, that it may be seen what reason I had to suppose it at 100,000, I have brought my pear-gage filled in this last experiment, for the inspection of the gentlemen present.

EXPERIMENT XV.

The same piece of leather used in the eleventh experiment was put into a damp cellar, where it was left till the

the next day; it was then put again into the receiver, and the degree of exhaustion, according to the barometer-gage, was 300, and according to the pear-gage 3500.

Being now perfectly satisfied that the variation in the testimony of the pear and barometer-gages was occasioned by the moisture contained in the substances I had put into the receiver assuming the form of vapour; I determined next to try what would be the effect of the vapour which might arise from small quantities of different fluids, and from some other substances containing moisture of various kinds.

Substances put into the receiver.	Weight when put in.	Degree of exhausting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XVI. Water in a watch-glass,	3 grains	148	24,000	lost $1\frac{1}{2}$ grain.
EXP. XVII. Water in a glass cup, } diameter two inches, —	100 grains	89	8000	lost 2 grains.
EXP. XVIII. Spirit of wine in } the same cup, —	100 grains	54	6000	lost 9 grains.
EXP. XIX. Vitriolic acid, —	100 grains	340	220	gained 1 gr.
EXP. XX. A piece of the inside } of a china orange with some } of the rind, —	100 grains	160	100,000	lost $2\frac{1}{2}$ grs.
EXP. XXI. A piece of the inside } of an onion, —	100 grains	160	100,000	lost $1\frac{1}{2}$ grain.
EXP. XXII. A piece of tainted beef,	100 grains	152	100,000	lost $2\frac{1}{2}$ grs.
EXP. XXIII. A piece of fresh beef,	100 grains	136	100,000	lost $2\frac{1}{2}$ grs.
EXP. XXIV. Spirit of turpentine,	100 grains	301	1800	lost 2 grains.
EXP. XXV. Pearl-ash, —	2 ounces.	118	5000	
EXP. XXVI. The same pearl-ash } made very hot, —		198	420	
EXP. XXVII. A lighted candle } held in the receiver till it went } out, — —		297	1800	

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Substances put into the receiver.	Weight when put in.	Degrees of exhausting according to		Change in weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XXVIII. A piece of charcoal,		129	1800	
EXP. XXIX. The receiver heated by holding several pieces of lighted charcoal in it, and then the above piece being thoroughly lighted was put into the receiver, and the pump worked, —		650	600	
EXP. XXX. Camphire, —	100 grains	304	520	{ lost barely $\frac{1}{2}$ a grain.
EXP. XXXI. Sulphur made to burn on a piece of brass, —		247	320	

Observing by these experiments that the small quantity of moisture which exhaled from the substances under the receiver prevented the pump from exhausting it to any very considerable degree, I began to suspect that whenever wet leather had been used to connect the receiver with the plate, there must have risen so great a quantity of vapour as to have prevented the degree of exhaustion from being near so great as in some of the foregoing instances. These suspicions induced me to make the following experiments.

	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
EXP. XXXII. The receiver was taken off, and after the cement was wiped clean from it, and every part made perfectly dry, it was put again on the pump-plate, and a little oil only was poured round the outside edge,	nearly 600	full 600
EXP. XXXIII. The receiver was taken off again, and instead of the oil it was set on a piece of leather, which had been soaked two days in water, —	51	16,000
EXP. XXXIV. The last experiment repeated with the same piece of leather, — —	51	1500
EXP. XXXV. The last experiment repeated again with the same piece of leather, — —	51	1000
EXP. XXXVI. The receiver was taken off, and instead of the leather soaked in water, there was put on a piece of the same sort of leather, soaked in a mixture of water and spirit of wine, such as Mr. SMEATON used,	47	12,000
EXP. XXXVII. The last experiment repeated with the same leather, — — —	47	1150
EXP. XXXVIII. The last experiment repeated again with the same leather, — — —	47	500

The great difference in the testimony of the pear-gage in these six last experiments appeared to me exceedingly astonishing, for the leathers seemed each of them to be as moist at last as at first.

By these experiments I was convinced how effectually the use of leather soaked in water, or in water and spirit of wine, prevents the pump from exhausting to any considerable degree. I have made a number of experiments of the same kind as these; but have never been able to exhaust, under such circumstances, to a greater degree than between 50 and 60, when the heat of the room was

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about 57° by a thermometer of FAHRENHEIT's scale: but the following experiments will shew how much some different degrees of heat affect the degree of exhaustion.

	Height of the Therm.	Degrees of exhaustion according to	
		Barom. gage.	Pear-gage.
EXP. XXXIX. Receiver set on leather which had lain all night in water, —	46	84	20,000
EXP. XL. Receiver set on a leather soaked all night in two parts water and one of spirit of wine, — — —	46	76	8000

The pump having been put in a room of the heat of 57° of FAHRENHEIT's scale for seven hours together, with the leathers put in the same water and the same spirit of wine and water which they had been soaked in all night, and which had been used in the two last experiments, the following experiments were made.

	Height of the Therm.	Degree of exhaustion according to	
		Barom. gage.	Pear-gage.
EXP. XLI. The receiver set on the leather soaked in water, — —	57	56	16,000
EXP. XLII. Receiver placed on a leather soaked in water and spirit of wine, —	57	49	1200

The following table will shew the comparative excellency between the pump on Mr. SMEATON's principle with which the chief of these experiments have been

tried, and one of my common double barreled table air-pumps under the same circumstances. The leather on the pistons of both was soaked in oil and tallow, and the receiver cemented down to each plate; the pumps were both of them fresh oiled.

	Pump on Mr. SMEATON'S principle. Degrees of exhaustion according to		Common pump. Degrees of exhaustion according to	
	Barom. gauge.	Pear- gauge.	Barom. gauge.	Pear- gauge.
EXP. XLIII. A piece of leather, weighing 100 grains, as it came from the leather-fellers, was put into the receiver of each pump, both pieces being cut from the same skin close by each other, —	152	100,000	108	12,000
EXP. XLIV. The same pieces of leather dried by the fire till they would lose no more of their weight, — —	506	520	160	165

The following experiments will shew the effect of water used in the barrels of pumps to make the pistons move air tight in them.

I took the same common air-pump used in the last experiment, and having taken off the leathers soaked in oil and tallow from the pistons of this pump, and wiped the barrels as clean as possible, I then put new leathers, which had been soaked in water, and new bladder valves; the receiver was then cemented to the pump-plate as before.

	Degrees of exhaustion according to	
	Barom. gage.	Pear- gage.
EXP. XLV. The pump was then worked as usual, —	37	38
EXP. XLVI. The last experiment repeated with another common pump, the leathers of the pistons of which were also soaked in water, —	34	37

From these experiments it evidently appears, that the air-pump of OTTO GUERICKE, and those contrived by Mr. GRATORIX, and Dr. HOOKE, and the improved one by Mr. PAPPIN, both used by Mr. BOYLE, also HAUKS-BEE'S, S'GRAVESANDE'S, MUCHENBROOK'S, and those of all who have used water in the barrels of their pumps, could never have exhausted to more than between 40 and 50, if the heat of the place was about 57; and although Mr. SMEATON, with his pump, where no water was in the barrel, but where leather soaked in a mixture of water and spirit of wine was used to set the receiver on the pump-plate, may have exhausted all but a thousandth or even a ten thousandth part of the common air, according to the testimony of his pear-gage; yet so much vapour must have arisen from the wet leather, that the contents of the receiver could never be less than a seventieth or eightieth part of the density of the atmosphere: nevertheless, it does not seem that any deficiency in the construction of Mr. SMEATON'S pump was the cause of his

not being able to exhaust beyond the low degrees of 70 or 80. Had he been aware of the bad effects of setting the receiver upon leather soaked in water and spirit of wine; and had he made use of the precaution to free all parts of his pump as much as possible from moisture, I make not the least doubt but the air-pump, which he executed himself, would have exhausted to as great a degree, as that pump has been seen to have done with which the chief of these experiments were made.

Having read the principal part of this paper to Mr. SMEATON, and shewn him some of the experiments; one in particular, where the pear-gage, as he observed himself, was filled to no less than 100,000th part of the whole content; he remarked from memory, that he had in several trials exceeded 1000 times, and once, as he remembered, near or about 10,000 times; but as he never could account how this happened, which appeared to him perfectly accidental, and therefore could not depend upon doing it at pleasure, he contented himself with putting down 1000 times, as being what (under the circumstances mentioned in his papers) he had a tolerable certainty of.

I must here again observe, that if we only wish to know the quantity of permanent air remaining in the receiver after it is as much exhausted as possible,

ble, it seems, that it is by Mr. SMEATON's gage only that we can know it. Again, when by the assistance of his gage and the barometer-gage together, we have discovered that there is a vapour which arises and occupies the place of the permanent air which is exhausted, it seems that it is by the means of his gage only that we can discover what part of the remaining contents of the receiver consists of this vapour, and what part of permanent air.

An account of some further experiments made with the same air-pump on Mr. SMEATON's principle, the results of which were different from the former.

AFTER I had made the foregoing experiments, and thought to have done with the subject (for some time at least) in reviewing them for the last time, I perceived one or two, the extraordinary results of which (though not unnoticed by me before) I now thought I had not paid sufficient attention to. Experiment the 19th (in which I found that when vitriolic acid was put into the receiver, and the pump worked for the usual time, the pear-gage indicated a much less degree of exhaustion than the
barometer-

barometer-gage) seemed to me now so surprizing, that it was impossible not to wish to repeat this experiment with all the care possible, and to endeavour to recollect all the circumstances which I thought could any way influence the result.

The vitriolic acid I made use of in the following experiments was some that I had had by me for some time; it had been kept in a phial, stopped with a glass-stopper, and tied over with a bladder. The thermometer in the room was at 59° , and the weather remarkably dry.

	Weight when put into the receiver.	Degrees of exhaustion according to		Variation of weight during the experiment.
		Barom. gage.	Pear-gage.	
EXP. XLVII. Vitriolic acid in a glass cup, two inches diameter, }	100 grains	602	380	gained 1 gr.
EXP. XLVIII. The last experiment repeated with the same vitriolic acid in the same cup, }	101 grains	502	350	gained $\frac{1}{2}$ a gr.
EXP. XLIX. The former experiment repeated again with the same vitriolic acid in the same cup, }	101 $\frac{1}{2}$ grs.	502	350	
EXP. L. The former experiment repeated the fourth time in every respect as before, }	101 $\frac{1}{2}$ grs.	502	340 +	gained a $\frac{1}{4}$ of a grain.

The generation of vapour in the exhausted receiver which Mr. CAVENDISH had supposed, and which I seemed, by my former experiments, to have proved, appeared satisfactorily to have accounted for the pear-gage's

gage's indicating a greater degree of exhaustion than the barometer-gage: but what to suppose could possibly make it indicate a less, I was entirely at a loss; for after having made these surprizing experiments on the vitriolic acid, I wished once more to try if I could repeat, with the same result, some of my former experiments, in which the pear-gage had indicated so nearly the same degree of exhaustion as the barometer-gages. Accordingly I took away the vitriolic acid from under the receiver; the pump was then fresh oiled, and I was very careful to wipe clean and dry the receiver and pump-plate, and then cemented down the receiver as usual.

EXPERIMENT LI.

The pump was then worked, and the degree of exhaustion appeared by the barometer-gage to be 432; but by the pear-gage to be but 370.

I know of no circumstance attending this experiment that differed from those in which my former experiments were made when the gages agreed so nearly, unless it were that of the weather: I recollect that it was then very damp, and now it had been very dry for some time. How this circumstance could make so great an alteration in the result of these experiments, I cannot pretend.

tend to say; but some of the following experiments will shew that the pear-gage still continued in many cases to indicate a less degree of exhaustion than the barometer-gage.

In all the preceding experiments the pump was worked for ten minutes, and the pear-gage was at the end of that time pushed down so as for the mouth to be immerfed in the cistern of quicksilver, and the air then let in according to the manner of using this gage; but now that I found that the testimony of this gage so seldom agreed with that of the barometer-gage, I wished to try, whether they might not agree when the receiver was exhausted only in part, though they did not when it was exhausted as much as possible. For this purpose I had the same receiver fitted with two pear-gages, so that I now could immerse the mouth of one of them in the cistern of quicksilver when I had exhausted the receiver in part only, and not immerse the mouth of the other till the receiver was exhausted for the usual time of ten minutes; in which time I found I could always raise the quicksilver in the barometer-gage as high as if I were to work the pump much longer.

	Degrees of exhaustion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear- gage.	Barom. gage.	Pear- gage.
EXP. LII. The receiver cemented to the pump-plate, —	430	300	430	360
EXP. LIII. The receiver unce- mented, wiped clean from the cement, and put on the pump-plate, with a little oil round the outside, —	502	360	502	360
EXP. LIV. The receiver put on a leather soaked in oil and tallow,	502	320	323	500

This last experiment seemed very extraordinary; for after having worked the pump for five minutes only, the barometer-gage indicated a degree of exhaustion of 502; but by working the pump five minutes more, it indicated a less degree of exhaustion, *viz.* 323. This effect I have observed to happen more than once.

	Degrees of exhaustion when the pump had been worked			
	Five minutes; according to		Ten minutes; according to	
	Barom. gage.	Pear- gage.	Barom. gage.	Pear- gage.
EXP. LV. Receiver put on a leather soaked in water for one night,	47	380	63	8000
EXP. LVI. Receiver put on a leather soaked for one night in a mixture of two parts water and one spi- rit of wine, — —	48	300	50	1200

The receiver was then taken off from the pump-plate, and a blank screw screwed into the hole in the pump-plate.

E X P E R I M E N T LVII.

The pump was then worked for ten minutes without any receiver on the pump-plate. The barometer-gage alone being afterwards connected with it, the gage indicated a degree of exhaustion of 50 only, which was the same as in the last experiment; so that the bad effects of the mixture of the spirit of wine and water still continued.

I then poured about two spoonfuls of oil down the hole in the pump-plate, and the pistons were worked gently till most of the oil had passed through the pump into a reservoir made to receive it. A blank screw was then screwed into the hole in the pump-plate as before, and after the pump had been worked for a minute or two, four or five times, the air being let in between each time, the oil had then washed so much of the moisture out of the inside of the pump, that I was now able to exhaust to 430 instead of only 50 times, as before the oil had been made to pass through it.

During the course of these experiments on the air-pump it appeared, by the testimony of the pear and baro-

meter-gages, that the remaining contents of a receiver, when exhausted as much as possible, was at different times of different kinds; sometimes it seemed to consist entirely of permanent air, as when a little vitriolic acid, &c. was put in the receiver; and sometimes mostly of vapour arising from moisture, and but a very small proportion of permanent air, as when a bit of damp leather, &c. was in the receiver. I was now therefore desirous of seeing what appearance the electric matter would exhibit in these different rarified media.

For this purpose I had a glass tube made, of an inch bore, and four feet and a half in length. This tube was connected to the receiver of the air-pump by means of an elbow-piece of brass, to which it was cemented; which elbow-piece was inserted perpendicularly in the top of the receiver: as the elbow made a right angle, the tube itself was of course horizontal.

Moreover, at that end of the glass tube which was cemented to the brass elbow-piece, there was fixed, on the inside, a piece of brass wire, about three inches and a half long, filed to a point, and pointing towards the other end of the tube. At the other end of the tube was cemented a brass screw fitted to a brass cap, which screwed on it; and in this brass cap was fixed a brass wire, three inches and a half long, which pointed towards

the brafs point; at the other end of the tube, and at the end of this wire, was a brafs ball, eight-tenths of an inch diameter.

The brafs cap at this extremity of the tube which is fartheft from the receiver was made round, and placed fo as to be in contact with the prime conductor of an electrical machine.

I now firft put fome vitriolic acid into the receiver, as a means of being able to make the remaining contents of the receiver, when exhausted as much as poffible, to confift of permanent air only, unadulterated with vapour; and as the receiver was the fame I had ufed in my foregoing experiments, there were two pear-gages fitted to it, which pear-gages I pushed down into the cifterns of quickfilver at different times, and the pump was worked as in my former experiments for ten minutes: heat of the room 59°.

The electrical machine was worked during the whole of the experiments.

EXPERIMENT LVIII.

Electrical appearances exhibited.	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
Light began first to appear in flashes, — —	5	
Light appeared the whole length of the tube in stræ,	8	
Tube was filled with an uniform body of pale light,	74	75
The pump had now been worked five minutes.		
The pump was then worked five minutes more.		
The tube was still filled with a uniform body of pale light, — — — — }	269	230

The conductor being then removed to a distance from the tube, it was made to approach it by degrees till a spark struck it, which was at the distance of two inches; the light in the tube now appeared like a compact body of fire, of a vivid purple colour, tending to a red.

Objects were seen through the tube when filled with this body of the electrical light, no less distinctly than if there had been no such light in the tube^(a).

The vitriolic acid being taken out of the receiver, I put a piece of leather of 100 grains, as it came from the leather-sellers, into that end of the tube which was next to the conductor of the electrical machine, and farthest from the receiver of the air-pump. I put the leather in

(a) This circumstance has been before remarked by Dr. HAMILTON in his conjectures on the tails of comets.

at this end of the tube rather than into the receiver, to be sure that the tube might be filled with the vapour arising from the damp leather rather than with the common air. The pump and electrical machine were then worked as before.

E X P E R I M E N T LIX.

Electrical appearances.	Degrees of exhaustion according to	
	Barom. gage.	Pear-gage.
Light began first to appear in flashes, — — —	12	100
Light appeared the whole length of the tube in striæ,	22	
Light vanished scarce to be seen, — — —	90	
The pump had now been worked seven minutes. The conductor was now removed from the tube, and the greatest striking distance was found to be one inch,		
The tube now appeared luminous, but the light was faint and white. The conductor was then again put in contact with the tube, and the machine worked: the pump was also worked for three minutes more, but scarce any light appeared, — — —		
	148	20,000

The conductor was then again removed from the tube, and the striking distance was found to be only one inch and four-tenths: the tube at the time of striking was luminous as before, and the light was of the same faint white colour.

Having lately received from my friend Dr. LIND some æther prepared by the ingenious Mr. WOLFE, I was very desirous to try whether I could produce any considerable degree

degree of cold by the evaporation of æther under a receiver whilst exhausting. For this purpose I put the æther into a phial, the neck of which was sufficient to admit the ball of a thermometer: this being placed on the air-pump, under a receiver which had a plate at the top, with a wire passing through a collar of leathers; to this wire the thermometer was fixed, by which means I could easily dip the ball of the thermometer into the æther.

EXPERIMENT LX.

The pump was now worked, and whilst the receiver was exhausting, the ball of the thermometer was often dipped into the æther; and when the degree of exhaustion by the barometer-gage was 65 (which was the utmost in this case that the pump would exhaust to) the degree of cold indicated by the fall of the quicksilver in the thermometer was 48° below 0 on FAHRENHEIT'S scale; so that there was a degree of cold produced 103° colder than the air in the room where the experiment was made, the thermometer in it being at 55° degrees above 0. The pump was kept continually working for half an hour, and the ball of the thermometer often dipped into the æther; but no greater degree of exhaustion or cold could be produced. The air being let into
the

the receiver, the quicksilver in the thermometer rose 10° , viz. to 38° below 0.

E X P E R I M E N T L X I.

Fresh æther being put into the phial to what was remaining, the thermometer rose to 30° above 0: the pump was then worked again constantly for half an hour; yet by the barometer-gage the degree of exhaustion was now not more than 16, and the degree of cold produced did not exceed the 11th degree below 0, as appeared by the quicksilver in the thermometer. The air being let into the receiver, the remaining æther was examined, and there were found several pieces of ice at the bottom of the phial, some of them as big as large peas, which, when the æther became nearly of the heat of 32° or freezing point of water, were intirely dissolved.

The air-pump with which these experiments were made exhausted above 400 times before the æther was put under the receiver.

